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CHLORITE OF POTASSA.

On the Chlorite of Potassa. By **William Tully**, M.D. Prof. Mat. Med. and Therap. in Yale College.

[Concluded from p. 333.]

In my view, then, the operation of this salt, as manifested in the preceding experiments, plainly appears to be (in kind, but not in degree) precisely like the effects of nitrate of potassa. In several instances, in chronic diseases, that were nearly if not quite local (as, for example, in a case of stricture of the urethra, mistaken by the physician in attendance for lithia, and, what is quite as singular, treated by a free use of nitrate of potassa), I witnessed the effect of that salt continued for a long time in efficient quantities, but not in so large doses as to produce any immediate inconvenience. In several instances, also, I have known two or three drachms of it, and in one instance probably about four drachms, taken by mistake, and not subsequently discharged, either by vomiting or purging; so that I well know its effects. From these observations, I should judge that the salt which is the subject of this essay possesses between a quarter and a third of the medicinal activity of the nitrate of potassa.

Again: the operation of this salt appears also to be precisely similar (not only in kind, but even in degree) to the effects of the bi-carbonates of potassa and soda. My attention was first turned to the effects of these last salts by observing their operation upon certain individuals, in the form of the popular beverage called soda water, as it was prepared several years ago; and afterwards I made some experiments with one of them, in single full doses, or rather very large doses (upon myself), for the sake of obtaining further assurance that I labored under no mistake. The results of my observations and experiments, in regard to the bicarbonates of potassa and soda, and the nitrate of potassa, have been as follows:—

When used in a large quantity at once, or within a short period, though short of the amount that will occasion what is called actual poisoning, each of these salts produces a soft and feeble pulse—great flatulence, and often cardialgia—uneasiness and strong sense of load and weight, or distension, in the epigastrium, gradually increasing to a steady pain, like the beginning of colica ileus, with intolerance of pressure, progressively extending to the umbilical region, and even to the whole abdomen, subsequently becoming extremely acute and lancinating, and then considerably resembling the pain of colica rhachialgia—the tumefaction and tenderness augmented in proportion to the severity and continuance of the pain, and sometimes attended at last with severe vomiting, and more or less spasms. This, it will be observed, is thus far identically the operation of the salt under consideration.

When used in much smaller, but still efficient quantities, for a considerable length of time, the nitrate of potassa (and also the bicarbonates of potassa and soda) produces general debility, frequent soft and feeble pulse, pale countenance, a brown crust about the teeth, a peculiar diminution of the tone of the stomach and intestines, great flatulence and often cardialgia, utter loss of appetite, often great diminution of digestive power, sometimes intolerance of most sorts of food, occasionally frequent vomiting, and sometimes diarrhœa. When the perfect identity of the effects of a single large dose of the salt of which we are treating, with the effects of a similar dose of these salts, is considered, no reasonable doubt can remain that it is also capable of producing this last described group of effects.

But, as I have already said, in the earlier part of my medical practice, I employed this salt considerably in various diseases; and, as I have likewise stated, I have continued to use it occasionally (though rarely) up to the present time. Now, in true phlogistic diseases, I repeat, I think I have certainly witnessed an obvious and palpable antiphlogistic or reducing effect, and I am confident I have never witnessed any other medicinal operation from it; for I do not consider the slight increase of the urinary excretion, which it produces, as being, in fact, a medicinal effect, since it is too inconsiderable to be of any use in the treatment of disease. As I have stated, I have formerly tried this salt, in moderate quantities, in various diseases of the nervous function; and occasionally, when I told my patient what I hoped it would accomplish, he sometimes flattered himself that it did operate slightly in this way; but this has never been at all obvious to me, and even the patient has generally had some doubts of it. In other instances, where I did not explain the purpose for which I gave it, no effect has been perceived.

The tendency of this salt (when used to a certain degree of excess) to produce symptoms of colic and proctica marisca, though without cessation of the peristaltic action of the intestines, very strongly assimilates with all the unequivocally refrigerant salts, and even with the cathartic ones. In the eighth experiment, this article even seemed to prove cathartic, like the refrigerant salts; while, in the other experiments, it did not even produce a laxative effect; and yet, notwithstanding the purgation in the eighth experiment, which must have carried it out of the intestines, it operated, in other respects, much the most powerfully. Many practitioners, I doubt not, from the symptoms, would have considered further purging, rather than opium, as the appropriate remedy for the ill effects produced in this case. For myself, however, I entertain no doubt, from past observations in regard to nitrate of potassa, that purging merely would rather have aggravated than mitigated the evil.

It is my opinion, therefore, that, as respects medicinal powers, this article is a pure reducing, antiphlogistic, or refrigerant remedy; i. e. it directly reduces vital energy and strength of action in the arterial system, and of course relieves phlogistic diathesis. My colleague, Professor Ives, has formerly told me, that, according to his observations, this salt certainly diminishes the heat of the body. This corresponds perfectly with my observations in regard to its effects in the treatment of phlogistic diseases, and even in some cases where the heat has been purely irritative, not being connected either with entony (phlogistic diathesis) on the one hand, or any positive atony on the other. But I have not found it capable of abating all sorts of heat; for, in my early practice, I employed it somewhat in hot typhus, but rather with an aggravation than a mitigation either of the heat or any other morbid symptom. In this respect, it is perfectly analogous to nitrate of potassa.

I trust that it is unnecessary, at the present day, to show that the living human body cannot be oxygenised, like a piece of inanimate iron or copper, in the laboratory of a chemist; and, as it is now conceded that this salt is of no value in syphilis, it must be unnecessary to discuss its value in this point of view. If we search for proof of its efficacy in scurvy, I believe we shall fairly lose our labor, as I know of nothing upon the records of medicine that contributes at all to show this; and the same remarks appear to be equally applicable in regard to its powers in diseases of the skin, and also in spasmodic affections. That it has some diuretic powers, is certain; but these, as I have said, appear to be too trifling to be worthy of any consideration. It is, in my view, clearly febrifuge, but by no means in the same sense in which cinchona is febrifuge. Cinchona is febrifuge in intermittent and remittent, but not in cauma and the caumatoid phlogotica. Tartrate of antimony is febrifuge in cauma, but not in intermittent, or low malignant typhus. Now I think there is abundant reason to conclude that the febrifuge powers of this salt are in analogy with tartrate of antimony, and not with cinchona. It is to be regretted that physicians still continue to employ such vague terms as febrifuge, which scarcely convey any more precise knowledge, in regard to the particular virtues of a remedial agent, than the term medicinal. Under different circumstances, bleeding, antimony, nitrate of potassa, opium, cinchona, and alcohol, are all febrifuge. Now what other term is there, in the materia medica, that will comprehend a more heterogeneous group, so far as relates to their operation, and the cases to which they are adapted? I believe these observations are equally true of antemetics. What could possibly have been the origin of the opinion that this salt is a stimulant, I cannot imagine. When it was first introduced into the materia medica (as I have already stated), it was a prevailing theory, or rather hypothesis, that the refrigerants, or, in other words, antiphlogistics, were merely chemical, and not vital, in their operation, and that they produced their effects by supplying the system, through the medium of the digestive organs, with oxygen in a concrete state, thereby superseding the supposed necessity of its being received by the lungs, as was then believed, in a gaseous state, and, on its subsequent solidification, extricating the great quantity of combined heat, which was imagined to be instrumental in transforming it from a solid to a gaseous form. On this hypothesis, those articles which contained the greatest quantity of oxygen, in the loosest state of combination, ought to have been the most powerful refrigerant or antiphlogistic agents. Again: the salts with alkaline bases were supposed to be additionally refrigerant or antiphlogistic, by the "power they have of producing in the stomach a sensation of cold."* This article, then, being a salt of an alkali, and containing more oxygen, in a loose state of combination, than any other agent of the materia medica, ought to have been the most powerful refrigerant or antiphlogistic, instead of a stimulant. But if it is in fact refrigerant or antiphlogistic—i. e. if it possesses the power of directly reducing vital energy and strength of action in the arterial system—it cannot at the same time have a stimulant power; i. e. the power of producing a quickly-diffused and transient increase of vital energy and strength of action. It ought not to be forgot, in this connection, that this salt consists of six equivalents oxygen, one equivalent potassium, and one equivalent chlorine—and that nitrate of potassa consists also of six equivalents oxygen, one equivalent potassium, and one equivalent nitrogen; so that the two salts differ from each other only by one equivalent out of eight.

But Murray says, that, as a remedy, this salt may be classed with the nitric acid. Now I do not think that one single particular can be mentioned, in which it truly agrees with nitric acid. As far as has been ascertained, nitric acid is a moderate tonic and deobstruent, without any other powers, at least as an internal remedy; while this salt (according to my observations) would seem to be a pure refrigerant or antiphlogistic. I am aware that the mineral acids are commonly said to be refrigerants; but all those who call them refrigerant, call them tonic likewise. Now, if they are really tonic (of which I believe there is no doubt), how can they be refrigerant also? how can an individual article both increase vital energy and strength of action, and diminish vital energy and strength of action, at one and the same time? Is it not the fact, that, by the universal consent of the medical profession—a consent, doubtless, founded on experience—the mineral acids are considered inadmissible in truly phlogistic diathesis, and are thought useful only in atonic cases, which would not be the fact did they possess antiphlogistic, or, in other words, refrigerant powers?

Upon what ground, then, does Murray call this article tonic? The following would seem to have been his reasoning, viz.:—nitric acid is tonic; nitric acid imparts oxygen to the system; this salt also imparts oxygen to the system, therefore this salt is tonic: and so nitric acid and this salt are placed in immediate connection, in his classification of the materia medica; and nitrate of silver, all the mercurials, iron, zinc, copper, arsenic, bismuth, baryta, and lime, immediately precede nitric acid and this salt, in the same class, and in the order in which I have enumerated them. But whoever confides in such reasonings, as appear to be applied to this article by writers, will inevitably be misled. The powers of medicines, in all cases, must be ascertained by experiment or experience, and by actual observation of their use, and in operative quantities, too; and they cannot be ascertained in any other way. In the department of materia medica, "*no argument like matter of fact is.*"

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* Murray.

As I have placed the name *Chlorite of Potassa* at the head of this essay, it is necessary to make a few remarks, not only in explanation, but in justification, of the employment of a name not used in any of the common systems of chemistry. Chemical authors of the present period specify four compounds of chlorine and oxygen, the existence of all of which, I believe, is well established. The first is alleged to be composed of one equivalent of each of these elements, and it is denominated protoxyd of chlorine. In regard to the second, there has been a diversity of opinion. It is commonly said to consist of one equivalent of chlorine and four of oxygen, though the Count Von Stadion, one of its discoverers, supposed that it contained but three equivalents of oxygen. Under the former view, it has been called tetartoxyd of chlorine, and, under the latter, tritoxyd of chlorine. Dr. Thomson, in the first volume of the edition of his chemistry, which was published in 1831, considers it as a tetartoxyd; but, in the second volume of the same work, he says—"There can be but little doubt that Von Stadion's analysis is accurate, so that it is a compound of one atom chlorine, and three atoms oxygen." But he here considers it as an acid rather than an oxyd; and he says—"If it is placed in contact with an alkaline base, in its nascent state, a combination takes place, and a salt is formed, having a peculiar acrid taste, similar to that of the acid." He adds, that its "salts have the property of destroying vegetable colors." In the latter part of the same volume, where Dr. Thomson treats expressly of all the salts, he makes no mention of any salts of this supposed acid; but, from reference to his remarks upon what is called *Bleaching Powder*, and upon what is called the *Disinfecting Liquid of Labarraque*, under the article Calcium, in his first volume, we find that he imagines that a part of these compounds constitute his supposed salts of this supposed acid, as he there tells us that Berzelius considers the *Bleaching Powder* as a mixture of three atoms chlorid of calcium, with one atom of a salt of lime containing this supposed acid. Now, that Berzelius's view of the composition of the *Bleaching Powder* is not correct (though the elements of the compound will admit of it), is proved by the facts, that, when a strong acid is poured upon it, chlorine is extricated, which is not the operation of such an acid, either upon chlorid of calcium, or any salt of the oxyd of calcium, with an oxygen acid; and that it is not sufficiently deliquiating, when exposed to atmospheric air, to admit of its being three fourths chlorid of calcium, which is one of the most deliquiating compounds known. The acid character of the tritoxyd of chlorine does not, therefore, appear to me to be rendered at all probable. Of the two other compounds of chlorine and oxygen, the next consists of one equivalent of chlorine and five of oxygen, and the last of one equivalent of chlorine and seven of oxygen; and both are decided and unequivocal acids. But in the systems of chemistry there is a singular anomaly in regard to the nomenclature of these two acids; for, contrary to all rule, that which contains the least oxygen is called chloric acid (instead of chlorous), and that which contains the most is called per-chloric acid. This application of names undoubtedly originated from the circumstance, that the acid containing the least oxygen was discovered first, and then, being the sole known acid of chlorine with oxygen, was very properly called chloric acid; but, when another acid of chlorine containing more oxygen was discovered, the name of the first known should have been changed to chlorous acid, and the new one should have been called chloric acid. Such a change has actually been made in regard to the acids of selenium: for that which contains the least oxygen was first discovered, and was then called selenic acid; but, when that containing a greater quantity of oxygen became known, the name selenous was applied to the former, and the name selenic transferred to the latter. The same is the fact with regard to the cyanic and cyanous acids, the former denomination having been applied to the latter acid when that only was known; but when another was discovered, containing more oxygen, a change of names, as in the case of the acids of selenium, was immediately made.

But there are other objections to the prevailing denominations of the acids of chlorine, beside mere anomaly of nomenclature. At the present time, the salts of the peroxyds are distinguished from the salts of the protoxyds by the prefix *per* to the name of the salt. Thus, a salt of the chloric acid, with the protoxyd of iron or the protoxyd of mercury, is a protochlorate; and a salt of the chloric acid, with the peroxyd of iron or the peroxyd of mercury, is a perchlorate. But upon the present received nomenclature of the acids of chlorine, we shall have three sorts of perchlorates, of different composition, the distinction of which, these names do not make. By the retention of the now existing nomenclature of the books, in regard to the acid compounds of chlorine and oxygen, we shall be obliged to have perchlorates whose bases are protoxyds, and also two entirely distinct sets of perchlorates whose bases are peroxyds; i. e. perchlorates formed of different acids. Besides, judging from analogy, we may yet expect to discover an acid of chlorine and oxygen, intermediate between the two now known, and consisting of one equivalent chlorine and six equivalents oxygen; and likewise another, intermediate between the tritoxyd of chlorine and what is now called chloric acid, and consisting of one equivalent chlorine and four equivalents oxygen. As the now known acids of chlorine are at present called, it would be difficult to find names for the two new ones of the above description, at least in analogy with the nomenclature of other acids; but only call the now known acids of chlorine by the names of chlorous and chloric acids, and one of the *anticipated* new ones would be a hypochloric, and the other a hypochlorous, acid, in strict analogy with other existing cases.

But another reason may perhaps be assigned by some for the existing nomenclature of the books for the acids under consideration, viz. the fact that the bromic, iodic, and nitric acids contain just five equivalents of oxygen to one equivalent of their respective bases. I can, however, see no reason why the number of equivalents of oxygen contained in these acids should be considered the maximum quantity that shall form an *ic* acid, any more than the number contained in any other *ic* acid. According to the last edition of Thomson's Chemistry, the manganesic acid contains four equivalents of oxygen; the columbic, the tungstic, the molybdic, the selenic, and the sulphuric, contain only three equivalents of oxygen; the chromic, the arsenic, and the phosphoric, contain only two and a half equivalents of oxygen; the antimonic, the titanic, the uranic, the boracic, and the carbonic acids, contain only two equivalents of oxygen; the oxalic contains only one and a half equivalent of oxygen, while the telluric and the silicic acids contain only one equivalent of this element. Why, then, should the quantity of oxygen contained in one of these groups of acids be considered as the standard of an *ic* acid, any more than the quantity contained in any other group? Why is it not probable, from the analogy of chlorine, that acids of bromine and iodine, containing seven equivalents of oxygen, will yet be discovered?

From these considerations, I feel myself warranted in calling that acid of chlorine, which contains five equivalents of oxygen, by the name of chlorous acid; and that which contains seven equivalents of oxygen, by the name of chloric acid, which, of course, makes the salt of which I am now treating a chlorite of potassa. In adopting this nomenclature, I cannot but think that I am only anticipating the books for a short time.

By way of recapitulation, I will again enumerate the compounds of chlorine and oxygen, with their respective representative numbers, viz.:

1st. Protoxyd of chlorine, which is composed of 1 chlorine 36+1 oxygen 8=44.

2d. Tritoxyd of chlorine—1 chlorine 36+3 oxygen 24=60.

3d. Chlorous acid (now called chloric acid)—1 chlorine 36+5 oxygen 40=76.

4th. Chloric acid (now called perchloric acid)—1 chlorine 36+7 oxygen 56=92.

It will be observed that there is still room for the discovery of a deutoxyd of chlorine, consisting of 1 chlorine 36+2 oxygen 16=42; a tetartoxyd of chlorine (or, more probably, hypochlorous acid), consisting of 1 chlorine 36+4 oxygen 32=68; and a hypochloric acid, consisting of 1 chlorine 36+6 oxygen 48=84.

I shall only add, that the chlorite of potassa is commonly prepared by transmitting chlorine gas through a concentrated solution of the protoxyd of potassium, until (as is commonly but incorrectly said) the alkali is completely neutralised, or, better, till it is either decomposed or combined, so that the solution has no alkalinity. The *rationale* of the formation of this salt, by this process, seems to be as follows:—the protoxyd of potassium consists of equal proportions of oxygen and metal. Now, if six proportions of this oxyd, and six of chlorine, are employed, five proportions of the chlorine are believed to unite with five proportions of the metal, forming five proportions of chlorid of potassium; and the five separated proportions of oxygen are believed to unite with the remaining single uncombined proportion of chlorine, forming one proportion of chlorous acid; and this single proportion of acid is believed to unite with the single remaining proportion of the oxyd, to form one proportion of chlorite of potassa; so that the product must always be five proportions of chlorid of potassium, to one proportion of chlorite of potassa, without any remainder of any sort. Why all the chlorine does not unite with all the metal, and extricate all the oxygen (since the chlorine and the metal are supposed to have a stronger affinity than the oxygen and the metal), I cannot pretend to tell; for, with all our knowledge on this subject, the science of chemistry still abounds with inexplicable things of this sort.

New Haven, June, 1832.

REMARKS ON FEVER.

Some Remarks on Fever, and on the Prize Essay of the New York State Medical Society for 1828. By Joseph Comstock, M.D.

[Communicated for the Boston Medical and Surgical Journal.]

It was a wise remark of the father of his country, that the best method to preserve peace is to be prepared for war. In accordance with this sentiment, we may observe, that the medical world, in a time of general health, should be prepared to meet pestilence. The multiplicity of discrepant views, the extensive introduction of new terms, and the immense number of new publications, require a discriminating and generalising genius, like that of the illustrious Boerhaave, to reject the useless and concentrate the useful—to fix facts on a firmer foundation, and to establish paramount principles; not a mere compiler, but one that is able to supply some *new* links in the medical chain, which, even in this day of light, are still lacking, as well as to unite others, which, although well formed, remain still unconnected. He must be an anatomist, a physiologist, a pharmaceutist, and thorough in therapeutics. He must have the especial power of potent reason and acute discrimination, that he may be able to sweep away those numerous pleasing—sometimes elevated, but still more often unsubstantial—fabrics, which but puzzle, perplex, and often lead into the mazes of error. His pioneers must be those who have given their attention to one class of diseases; to one tissue of anatomical structure, healthy and disorganised; to one set of physiological actions, natural and morbid, and to the peculiar effects of a single remedy.

Half a century is now hardly sufficient to establish a single new article of the materia medica, owing to the want of specific attention, the omission of minute observation, and the lack of concentration of time and talent upon one single article.

With everything relating to *fevers*, he must be familiar; for he who knows the whole of their phases, phenomena, and treatment, is the most accomplished and useful physician. They are, in fact, the most extensive, the most variegated, and the most eccentric, of human maladies. He who knows best how to describe, define, and discover them, mitigate them when incurable, cure them when curable, and prognosticate their terminations, has arrived the nearest to the acme of his profession, and will find little difficulty in managing all other systematic complaints.

Still, fever, at this late day, is a name without a specified identity—a disease without a definition. This is not owing to any want of investigation, but to the intrinsic difficulties attendant on the subject itself. These difficulties do not apply, however, to the defining of any one particular fever.

Among the Greeks, *fire* and *fever* were nearly of the same import. Πυρ, *ignis*, fire, and hence pyrexia, is employed by HIPPOCRATES himself, to signify fever. The views of the ancients are some of them handed down by tradition, from periods of antiquity more remote than any manuscript now in existence. The universal idea conveyed by the term *fever*, both to the learned and unlearned, is that of heat, or an increase of temperature. And yet to this phenomenon there are striking exceptions; especially in fevers of the typhoid type, and their congeners. And such exceptions do not appear to have been unknown to HIPPOCRATES; for proof of which, the very term *typhus* is one. So different were *some cases* of fever from their common burning *form*, and yet so fixed was the opinion of *heat* and *fire*, as belonging to all, that a term was used, signifying that there was *fire* still, but that it was latent, hidden, smothered, or covered. Hence the origin of the word *typhus*, as applied to fever; the Greek word τυφω, as we are told by DR. GOOD, signifying "to smoulder, or to burn and smoke without vent." I know that some authors derive the term from τυφος, a noun, which means *stupor*. Stupor, to be sure, is sometimes a symptom of typhus; but, like spots, and fainting, and delirium, not uniformly so. The authority for the former Greek word, and its propriety, are, therefore, both immensely in its favor.

But the idea that all fevers are *hot* is the traditional and popular *one*, and the world never gives up its first impressions. With the multitude, or, in the language of DR. RUSH, with "the great vulgar and little vulgar," *first* impressions are *last* impressions; they are indelible. The idea, therefore, that all fevers are hot, is accompanied with others, viz. that all require cooling, and cathartics, and depletion. Where we find but few ideas, we always find those few very firmly riveted. Such is the case in this instance. Errors of some kinds may be few, but fatal. When typhus fever first became epidemic, in a certain district into which I was afterwards called, it was mistaken for worms. The attending physician gave cathartics of such kinds as are usual on such occasions. But such was the extreme debility of the little patients, that, when *these* began to operate, *they* began to die. It is believed that not one child lived who was subjected to this treatment. The destructive effects of the depleting mode of practice reached us from the navy, the army, and all parts of the United States. Previous to the invasion of typhus fever as an epidemy, and since its cessation, with the exception of sporadic or endemic cases, evacuations have been used, in the fevers of this country, with decided advantage. I well recollect a case of pneumonia, which ended in recovery, and which occurred about two years after the cessation of the typhoid epidemic. This case required no less than eleven bleedings, with other evacuations.*

On our accurate discrimination and correct diagnosis, sometimes hang life and death. It is highly interesting to the public, that accuracy should everywhere prevail; but errors are most to be lamented when they ema-

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*** This was an extraordinary case, originating, in a man, from a fall on the frozen ground, from a cart. I mention it as illustrative of the different states of the system, when typhoid diseases prevail and when they do not. During the reign of typhus, I saw two cases, both in men, who received bodily injuries less severe than this man, and both died with typhoid debility—the system, in neither case, never reacting at all!**

nate *from*, and prevail *in*, great states, great towns, and great minds. The state of New York—the first in our union in population, in the magnificence of its cities, in the fertility of its soil, and in the liberality and excellence of its institutions—should take the lead, also, in the correctness of its views. New York is the great source of reference and appeal in medical matters, at least in some sections of the union; and, as she is daily rising in glory, her influence and authority are daily increasing. How all-important, then, that SHE should be correct! At the head of her medical colleges, medical schools, medical societies, and medical professors, is *one*, called the STATE MEDICAL SOCIETY, holding its meetings in the capital city. Yet, so late as the year 1828, this society conferred its premium, for the best "ESSAY ON THE HISTORY, CAUSES, AND TREATMENT, OF TYPHUS FEVER," upon a writer who denies the animal origin of diseases, and the hurtful effects of putrefaction and filth in producing fevers!

So great a medical heresy was never promulgated from so high an authority. The whole medical world, HIPPOCRATES, GALEN, AARON OF ALEXANDRIA, RHAZES—all the names of celebrity, remote and recent—PRINGLE, CULLEN, CHISHOLM, WILSON, GOOD, and others, are thus set at nought! As to the history of typhus fever, we are informed, by him, that "the ancient authors on medicine make no particular mention of it" [p. 1]. According to this, HIPPOCRATES is not an ancient author; for he particularly mentions it, and from him the very name is derived.* Under the head of *Prognosis*, we are told that "the chief danger, not only in this, but in all fevers, arises from the disorganising effects of high and uncontrolled excitement," &c. [p. 7]. Will it be believed, by those who read this, that the writer ever saw a genuine case of typhus fever?

Of debility, he says—"This, then, is the case with the debility in the primary stages of typhus; it is increased by the exhibiting of stimulants, and lessened by evacuating measures" [p. 10]. Further, we are told [p. 11], that "every physician must have witnessed the positively strengthening effects of cathartics, in typhus fevers, before the constitution has been really debilitated by long-continued excitement."

A writer of this stamp, we may be assured, cannot get along without shedding much blood; accordingly [at p. 22], we have the following passage: he says—"We should draw blood *pleno rivo*, and not rest satisfied with a small dribbling stream; the first mode of bleeding is infinitely preferable to the last, as it effects a more speedy reduction of the pulse, and sooner brings on a state of fainting, which is much to be desired in very violent cases, as it is accompanied with vast relief to the inflamed organ." Will it be credited, that this sentence, which I have extracted entire, relates to the typhus fever of the United States, in which, "in very violent cases," the patient faints spontaneously, has a pale countenance, and a weak, tremulous, thread-like pulse, if any pulse at all? Yet such is the fact! The prize pamphlet lies before me, and I have compared my extract with every word and every letter of the original. This sentence has a note in the margin, from which I will extract three lines, in order to show the prize-writer's notion of typhus fever, and the disease by which he illustrates it. He says—"We have a striking example of the powerful effect of bleeding *ad deliquium*, or until a sensation of faintness or sickness is produced, in cases of violent ophthalmia."

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* See Latin translation of his works, by HALLER.

As adjuvants to bloodletting, Dr. Hamilton's purgative plan, and some pills of his own, formed of calomel, jalap, and aloes, are recommended. But, having discovered that cold bathing was debilitating, its aid is put in requisition, along with bloodletting and cathartics, to further reduce the pulse and prostrate the powers of life. Thus we are told [p. 37], that "these facts unquestionably prove that cold has none of the properties of a stimulant, since it weakens the pulse in a state of health, weakens it still more when already weak, as in a chill, and lessens very much its volume and frequency when in a state of excitement and fever."

As the writer does not allow cold to be a stimulant, he infers that it is a sedative, and says—"It is from its operating in this way, that we can readily account for its extraordinary effect in fever" [p. 38].

So intent is he on drawing blood in typhus, that, under the head of *Congestive Typhus*, which he considers the form putting on the greatest appearance of debility, he says—"If we cannot, however, procure blood from a vein, we must resort to arteriotomy" [p. 45]. We are told that "we are not to rely solely on bleeding, the bath, &c., in the cure of congestive fever. After bleeding," he says, "we should immediately order large stimulating enemata, that the bowels may, if possible, be at once evacuated, and then commence the use of cathartics." For this purpose, he again recommends calomel and jalap [p. 46].

Thus the twenty-fourth aphorism of Hippocrates* is trodden under foot of men, as well as women!

It cannot indeed be discovered, by this essay, what the difference between typhus and inflammatory fever consists in. There is no attempt to define the former; and, as to the treatment, we have seen that it is wholly adapted to the most intense grade of the latter.

As the writer is at odds with the great and good of the medical world, he can hardly get through his pamphlet without being at odds with Dr. Magill himself.† Having, in the anterior part of his essay, quoted the noted case of the fever at Oxford, in 1577, occasioned by the effluvia of a filthy prisoner, who was tried at the Black Assize, and who poisoned the court and spectators—and having referred to other similar instances at Exeter, Taunton, and the Old Bailey—*he*, in the latter part of his essay, adduces the opinions of Dr. Bancroft and himself, founded on the filthy manner of living of the Russian boors, the Greenlanders, the Esquimaux, the Samoides, &c., as triumphant proofs of the harmlessness of animal putrefaction and the eating of carrion! He ought to have called to remembrance the power of habit, which accounts for all these phenomena; and that Rowland Jenks, the criminal at the Black Assize, who poisoned the court and some five hundred others by the effluvia of his clothes and person, *was not himself affected.*

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* Sect. 1.—Women resorted to family physic, with very pernicious effects, in typhus fever.

† Alfred T. Magill, M.D., of Winchester, Virginia, author of the Prize Essay. New York, 1829. Charles S. Francis, 252 Broadway. pp. 69.

And now, once for all, we may with truth remark, that it is not one cause alone which produces fever. There is a predisposition to be affected in those who have fevers, and there is an idiosyncrasy to resist the most concentrated causes in those who escape. Hence, it was never known that the most *virulent virus* prostrated every single individual exposed to its influence. No; not even the plague, smallpox, or bite of a rabid animal. A predisposition to be acted on sometimes pervades a ship's crew, an army, a court, or a family. At other periods the majority escape, and one only, or a few, are attacked. In fact, an immunity is more common than an epidemic, although far less noticed. The pages of history record only battles, invasions, sieges, and massacres—sudden deaths, violent deaths, plagues, and epidemics. The times of peace, and plenty, and prosperity—of health, public and private, pass unheeded.

The plague of India, and the present pestilence of Europe, the *cholera*, is acknowledged to be a disease of the typhoid diathesis. It is treated by the celebrated MAGENDIE, at Paris, with hot wine and strong punch. It is, therefore, very important that typhoid diseases should be well studied, and their nature fully understood. He who thinks that he finds a typhus fever with a full, hard pulse, a tense fibre, and which requires repeated bleeding and drastic cathartics, is utterly mistaken. There never was, and never can be, any such a *typhus fever*. Neither ophthalmia, cephalitis, gastritis, nor carditis, are claimed to belong to its class, neither pneumonia* nor phrenitis to partake of its nature. If there is ever any inflammation attending typhus, it is of the erysipelatous kind, in which bleeding is death—*at least sometimes*. In a case of typhus fever, the most distinctly marked, with all the symptoms of complete debility, such as fainting when lying on the bed and on the back, the first attack was that of erysipelas in the face and forehead.

It would seem that every kind of fever has been called typhus by some indiscriminating writers. The author of the prize essay, speaking of the pulse, says, that "in some cases it is tense and hard." Now we deny the existence of such a pulse in typhus fever. It is known only by a soft, feeble, and easily compressible pulse, whether or not it be increased in frequency. In general, however, the pulse is very much accelerated, varying from one hundred and twelve to one hundred and forty in a minute. We apply the terms *typhus* and *typhoid* exclusively to diseases of direct debility. Cholera is undeniably a disease of this class; so are cynanche maligna and scarlet fever. Cynanche trachealis, or croup, on the contrary, is a disease of a contrary diathesis, the attending fever being inflammatory, or a *cauma*. The last case of this the present writer had, his little patient was saved by the very free extraction of blood by leeches applied to the throat, aided by emetics of ipecac., tartarised antimony, and lobelia.

These remarks on the prize pamphlet would not have been given the public, had not its doctrines been sanctioned by the high authority before mentioned, and been so utterly discrepant to our own views of the subject, founded on a large experience. They are made with a spirit of conviction of the danger of its doctrines, and not of hostility to any person or persons. The writer of it deserves the credit of wielding a potent and polished pen, which is fully conceded him.

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* Pneumonia typhoides is here, of course, excepted.

Lebanon, Ct., June, 1832.

BOSTON MEDICAL AND SURGICAL JOURNAL.

BOSTON, JULY 11, 1832.

CHOLERA AT NEW YORK.

After our last paper went to press, we had intelligence of the occurrence of several cases of cholera at New York. Its introduction into that city appears, so far as facts have come to light, to be wholly independent of contagion. How far its subsequent progress has been aided by this cause, we have no means of judging.

The Board of Health report every day, at 12 o'clock, the number of new cases and of deaths. On the 5th instant there were 20 cases and 11 deaths ; on the 6th, 30 cases and 19 deaths ; and on the 7th, 42 cases and 12 deaths. The progress and fatality of the disease, thus far, has been much less than could have been expected in a commercial city of 200,000 inhabitants.

At the suggestion of our City Council, the Consulting Physicians have designated Drs. Bigelow, Ware, and Flint, as a Medical Committee, to visit New York, and acquire and communicate knowledge of the disease as it there prevails. These gentlemen left the city on Sunday last, and are now engaged in the duties of their commission. The result of a deputation so wisely selected will be communicated, as soon as possible, to our readers. In the mean time, we offer below a letter from Dr. Paine, of New York, which contains some interesting details of the post mortem examination of two (and we believe the first two) cases of the disease that proved fatal in that city. The letter was addressed to a distinguished practitioner in this city, to whom we are indebted for the liberty of publishing it.

New York, July 3d, 1832.

Dear Sir,—The reports respecting the existence of cholera in this city are so contradictory, that you may be desirous of knowing the absolute truth ; but I am chiefly induced to make you this communication for the purpose of submitting to you the morbid appearances on dissecting two individuals who were supposed to have died of this disease, and the notes of which I took during the examination, which was conducted by Drs. Carroll and Ludlow, and myself.

The subject of the first case is the man who lived at the corner of Reed and Greenwich streets. I did not see him till about twenty minutes after death, and have derived my knowledge of his symptoms from his wife and his physician.

He was forty years of age, very robust and corpulent—pursuing the trade of pianoforte making—of intemperate habits, and had drank rather excessively on Sunday, the 1st instant, though was sober enough to accompany his wife to church in the evening. He had complained of a sense of tightness about the region of the stomach, for four or five days previously, but at no time of any pain in his head, before or during the disease. His appetite remained good till the noon of that day, when he ate moderately of fried fish and potatoes. Soon after his return from church, he retired to bed, remarking to his wife that he had an unusual commotion in his bowels, which occasionally interrupted his sleep till 12 o'clock, when he first suffered the attack of vomiting and purging, attended by great distress in the abdomen.

The house was immediately deserted by the other occupants, and no messenger could be obtained by his wife, to procure a physician, till five o'clock in the morning, at which time the patient was visited by DR. CAMERON. The doctor informs me that he found him violently affected with vomiting, purging, and most convulsive spasms; the features sunken and the eyes staring; the pulse insensible at the wrist, and the surface cold, and covered with a clammy sweat; the countenance black and terrific; tongue of a dark purple during the spasms, becoming opalescent as the spasmodic action abated; the fluid rejected was watery, consisting probably of the liquids he was permitted to drink; his dejections resembled rice water, of the consistence of cream. The faculties of his mind were unimpaired to the last, and, during the short intervals of ease, his muscular strength was not sensibly diminished. He passed no water after the arrival of the physician.

Small quantities of laudanum were administered, and the usual external applications made, with the effect of diminishing the symptoms generally for two or three hours, at which time reaction began to take place, and at ten o'clock he was bled to the extent of ten or twelve ounces. The symptoms, however, had been again increasing, and continued to become more violent till the time of his death, which took place about eleven o'clock, the patient having suffered about eleven hours.

The lividness disappeared from his skin about half an hour before death, and returned, particularly to the face, about an hour after death.

I first saw him about twenty or thirty minutes after he had expired. The features had recovered from their contraction, and, with the exception of the eye, presented no unusual aspect. But the eye was remarkably expressive of that influence which the disease has been supposed to exert on the nervous system, and its wild, glaring stare was startling to the spectator. It was looking forward, and was much exposed by the contraction of the eyelids.

The muscles of the body were rather rigid, and the surface very cold. Spasmodic contractions of the lower extremities were still taking place; and I am informed, by DR. CAMERON, that the legs continued to move occasionally for an hour and a half after his death. The tongue and inside of the lips exhibited a dead white, with a slight shade of blue. This color was independent of the coating, which did not exist to any remarkable extent. There were several livid patches on the thorax and shoulders.

The dissection was commenced in about an hour and three quarters after death. At this time the face had again become very livid, and the skin was discolored extensively in other parts. The eye retained its remarkable wildness, much of which was remaining four hours afterwards, when we finished the dissection. The muscles still slightly rigid, but no spasmodic motion could now be observed. The incision penetrated about three fourths of an inch of cutaneous fat, and I was surprised to find the muscles so florid in a case of so much obvious congestion.

On opening the abdomen, the stomach and large intestines appeared very much inflated—the small intestines only partially so. The stomach and small intestines were florid, denoting great vascularity of the internal coat. Large intestines natural. Mesentery and omentum loaded with fat. No turgescence of their veins, or of the veins of the diaphragm. No fluid in the cavity.

The stomach contained a pint of a fluid resembling a weak infusion of coffee. A bright redness existed in the upper portion of the stomach, occupying about one third of the organ, and terminating abruptly near the cardiac orifice. All the inferior portion was slightly livid, and, at the pyloric orifice, a livid redness began abruptly, and extended through the whole tract of the small intestines.

In the large intestines, the mucous membrane was natural. A large quantity of a fluid, resembling oatmeal gruel, and of a shade between that and rice water, existed throughout the intestinal canal. There was rather more than the usual quantity of mucus, resembling cream, though not so white.

Liver externally natural—perhaps rather lighter than usual; on making incisions, the black blood flowed more freely than in a healthy state of this organ. The gall bladder contained about an ounce of black bile. The ducts perfectly natural, the bile flowing freely through them. Spleen and pancreas natural. Bladder empty, but not particularly contracted. The veins of the right auricle of the heart rather fuller than usual. No turgescence of the coronary veins. Lungs blue, and turgid with black blood to a very remarkable degree. Cavities of the heart and venæ cavæ entirely empty. No fluid in the thorax. Brain engorged with black blood, and the ventricles contained four or five ounces of serum. We were not permitted to examine the ganglia and spinal marrow.

Case 2. We immediately proceeded to examine the case of Betsey, the black girl, who resided as cook at Mr. A. Thomas's, No. 185 Reed Street, about twenty rods from the house of Mr. G., and where she had lived for several months. Betsey was very temperate and regular in her habits, but a large feeder—of slender frame, but capable of her duties, and generally enjoyed good health; about 20 years of age. She attended church in the afternoon of Sunday, the 1st instant, and made no complaint till six o'clock of that evening, when she felt some sickness and pain at her stomach, and went to bed. At two o'clock, A. M., the family were awakened by her groans. The morning after the examination, I received from her attendants the following account of her symptoms:—She was found stiff with the cramp, and severely affected with vomiting and purging; complained of great distress over the whole body; eyes staring and sunken, and the features contracted. Fifteen drops of laudanum and a little camphor were given, which removed the vomiting in a few minutes. The same dose was repeated in an hour after. The purging, however, continued, and the spasms constantly recurred, attended by great suffering. Profuse sweating took place towards morning, when the surface became rather warmer. At 11 o'clock, she began to sleep, and was then left by her attendants till four o'clock, at which time she was found dying.

The fluid vomited and dejected is said to have resembled rice water. She had no physician. In the progress of the disease, a dose of salts was given, and occasionally wine and water.

The dissection was commenced at 15 or 20 minutes after death. Every muscle was violently contracted, and remained so, without any motion, however, for an hour and a half, when the examination was finished. Skin livid, and petechiæ numerously distributed, appearing with great distinctness on the schlerotic coat of the eye. Features rather sunken; expression of the eye remarkably wild and glaring, corresponding exactly with this appearance in the other case. The eyes were also very much exposed, and turned upwards.

The stomach and large intestines externally natural, and free from flatus. Small intestines livid. Veins of the omentum and mesentery more than usually full. Mucous coat of the stomach slightly discolored; scarcely a morbid appearance. This organ contained a small quantity of a fluid resembling rice water. The lividness of the small intestines existed in the serous membrane, the mucous coat being natural. These organs contained a considerable quantity of a yellow fluid. The large intestines were filled with a fluid exactly resembling rice water, with flocculi like "half-dissolved flakes of snow." Mucous membrane natural. The mucus, which resembled a creamy substance, did not exceed the natural quantity.

Liver very purple—the left lobe in a plethoric state, and the right lobe very turgid, with black blood. The gall bladder contained about an ounce of yellow bile. The ducts perfectly natural. Spleen and pancreas natural. Urinary bladder contracted to a small, hard knot, evidently owing to the spasm. No fluid in the cavity of the abdomen. The lungs were found in a perfectly natural state. The heart was engorged with black blood, as were also the venæ cavæ. No fluid in the thorax. We were too much fatigued to enter on the examination of the brain, &c.

July 4.—Cases of this disease are now constantly occurring, and it attacks all ages from infancy. The symptoms are uniformly such as I have described. Dr. Bliss informed me to-day that he witnessed the dissection of a child, in which the morbid appearances were exactly such as I have described in Betsey's case. The invasion is very sudden, and without any premonitory symptoms. I believe some have recovered. They generally die in about 12 hours from the attack. It has now appeared in almost every part of the city, and no one thinks of tracing it to contagion. The vomiting is easily allayed by small doses of laudanum. The spasms are obstinate. The collapse is immediate, and no reaction takes place. In short, it is *death* at the very invasion. The probable number of deaths to this time, 6 P. M., is from 30 to 40.

Very respectfully yours,

To Dr. J. C. Warren, Boston. Martyn Paine.

Valuable Communications are again excluded by want of room.

Whole number of deaths in Boston for the week ending July 7, 32. Males, 14—Females, 18.

NOTICE.

The Copartnership heretofore existing between the Subscribers, under the Firm of JARVIS & PEIRSON, is dissolved by mutual consent. N. Jarvis will settle the affairs of the late Firm.

Nathan Jarvis.
George W. Peirson.

NATHAN JARVIS will continue the Business of Manufacturing and Selling *Drugs and Medicines*, at 188 Washington Street. Physicians and others will be supplied with Choice Medicines on reasonable terms. July 11.

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